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CONTENTS

Psychology and the Scientific Method: PAUL HORST..... 337

Are We Machines? And What of It?: W. H. ROBERTS..... 347

Book Reviews:

Rignano's The Nature of Life: W. H. SHELDON..... 356

Jennings' The Biological Basis of Human Nature: L. L. BERNARD.... 359

Lighthall's The Person of Evolution: D. MAURICE ALLAN..... 362

Journals and New Books..... 363

Notes and News..... 364

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THE JOURNAL OF PHILOSOPHY

PSYCHOLOGY AND THE SCIENTIFIC METHOD

A GREAT deal has been written about the scientific method in psychology. Unfortunately psychologists have not always fully appreciated the fundamental characteristics of this method. The philosopher, it appears, if we are to judge from the meagerness of his contribution to the progress of a scientific psychology, has failed to grasp and emphasize those characteristics of the scientific method which have made it so productive in the physical sciences during the past few centuries.

As for the physicist himself, the author and master of the method, he has been so thoroughly fascinated by the trees to the exclusion of the forest that he has not been able to tell the psychologist much about the forest. And the psychologist, looking with admiration if not envy across the fertile fields of physical research, has not been notably successful in identifying the factors contributing to that coveted fertility. He has erred often in assuming that the essence of the method consists in the employment of instrumental technique, not realizing apparently that the instruments were only tools of the method.

What, then, is the scientific method? Before an answer may be attempted we must first formulate the purpose of science. Directly or indirectly science has as its purpose the control of events. And such control is made possible by a knowledge of the sequential relationships of certain events. We do not say cause and effect relationships, for, as we shall point out, the results obtained by the use of the method imply no force or necessity in the nature of the sequences. Science, then, is primarily interested in acquiring knowledge concerning the sequential relationship of events. This statement offers nothing original or novel. It is one which is often made by both the philosopher and the scientist. But as it stands it is not of much value, for although the layman might think he knows what is meant by an event the term to the scientist is hazy and not very useful. Let us then proceed to define an event as a change. What it is that changes we shall discuss later.

If now we substitute this definition in our formulation of the purpose of science we may say that the aim of science is to discover the sequential relationships of changing conditions. And, having given

the aim or purpose of science, we are ready to inquire how this aim is achieved, that is, what it is that characterizes the scientific method. Being concerned with changes, the first task of science is to discover what it is that changes. The mathematician has given us a name for these changing entities. He calls them variables. The scientist then must first identify his variables. In physics this is relatively easy. As a matter of fact, most of his variables are readily identified, such as, pressure, volume, weight, resistance, temperature, etc. The variables involved in the phenomena studied by the psychologist are much more complex and hence more difficult to identify or to define conveniently and usefully.

But these variables are not free floating detached entities. They are characteristics or qualities of an object. Thus the physicist finds his variables in the objects which he studies. A description of an object therefore involves the enumeration of the variables in it. And here we may pause to point out the intimate relationship between the phenomena of the physicist and those of the psychologist. First let us ask what criteria are used in identifying the variables of a given object. In general a variable is isolated because it affects a given sense modality. We may take for example several of the familiar variables of physics and refer them to their corresponding sense modalities, such as force and kinesthesis, temperature and cutaneous thermal sensitivity, sound and audition, light and vision, etc. Since spatial attributes or variables like distance and volume are of more general significance they will be reserved for a separate discussion.

The correspondences indicated above briefly emphasize the thoroughly subjective character of the physicist's phenomena; that is, the fact that all his data are derived merely from sense impressions. This fact was emphasized by Karl Pearson a quarter of a century ago. "Science," says the great methodologist, "deals with conceptions drawn ultimately from sense impressions, and its legitimate field is the whole content of the mind."[1] Our purpose in here calling attention to the fact is not to disillusion the materialistic physicist, nor to inspire the floundering psychologist with the thought that after all his phenomena are no less tangible or accessible than those of his more successful colleague in the so-called physical sciences. Rather we take pains to point out the subjective character of the physicist's data because this fact is intimately related to the subject of spatial variables, which subject we shall discuss presently.

But first the concept of the object in which reside the varying attributes needs further clarification. The term "object" has perhaps not been advisedly selected. Here it is not used in opposition

[1] Pearson, K., *Grammar of Science*, p. 66.

to subject in the sense that a subjective and objective dichotomy of universes of discourse is implied. More appropriately we might use the term "structure" or borrow the word "system" directly from the physicist. The physicist employs the term in exactly the same manner as we have used the word "object." He speaks of isolating a system. And by isolation he means describing it according to certain criteria. We may take a typical example for illustration. Suppose we wish to find the law of tension in a rope which is wound round a post and is just on the point of slipping. We have to do here with a flexible inextensible weightless string wound on a rough circular drum. Consider an arc of the rope. Imagine this arc to be frozen so that we have a rigid body to deal with. We now isolate this system. That is, we describe the magnitude and direction of the forces involved, which are the specific variables in this system.

In general a system may be any complex of variable attributes, although the boundaries of the system are not always easily defined. Sometimes they may be arbitrarily determined. Usually contiguity and spatial localization of the variables is taken as the criterion by which they are regarded as belonging to a unit system or structure. For example, the fact that roundness and whiteness and hardness are all localized in a given position in space is adequate justification for regarding the complex as a unit structure, to wit, a golf ball. This illustration may appear absurdly obvious, but in the study of psychological phenomena logical correlates of the same criteria must be used when defining a unit system, and here the boundaries of the system are much more vague, though none the less important. The structure or unit system in which the psychologist finds his variables involves, of course, the individual organism. But it also involves his environment, so that the unit system is composed of certain variable aspects of his immediate environment as well as his variable responses.

To summarize briefly our discussion thus far, we may say that science is concerned with the control of events. Such control is made possible by a knowledge of the sequential relationships of changing conditions where these changing conditions take place with reference to given variable attributes of a bounded system. The first task of science is to bound the system by describing or defining the variables within it.

With this summary before us we are in a position to recur to the subject of spatial variables. It may be pointed out at once that what the physicist has regarded as objective realities are no more than spatial, or what amounts to the same thing, quantitative correlates for other variable sensory phenomena such as weight, sound, light,

etc. He speaks somewhat naïvely of light waves and sound waves as though they were something quite apart from the sensory phenomena with which they correlate. As a matter of fact, it is not the so-called objective character of the air or ether waves which has made the wave theory of such importance in physics, but rather the fact that waves have length and amplitude, and are therefore amenable to that most general, rigorous, and systematic treatment of which the human organism is capable, namely, mathematical analysis.

But we are not yet quite ready to indicate why the sort of correlates to be found for variable attributes should be of such a character as to be amenable to mathematical manipulation. First we must inquire more precisely into the nature of a variable. This brings us to the concept of continuity, a concept the utility of which has not always been appreciated by the psychologist, and not always properly emphasized by the physicist and the philosopher. We may illustrate this defect by indicating several types of inductive methods described by the philosopher and by citing stock illustrations of each. One of these methods is known as the "Method of Concommitant Variation" and may be thus defined; "If an antecedent and a consequent vary concomitantly, although no other relevant circumstance has changed, then that antecedent is a condition of that consequent."[2] As an example may be suggested the relation between the temperature and volume of a substance.

Another of the inductive methods as conventionally given is the "Joint Method of Agreement and Difference." "If a group of several instances in which a phenomenon occurs have nothing relevant in common except the absence of that antecedent, then that antecedent is a condition of that phenomenon." As an example, "Darwin observed that many plots of land containing all of them plenty of earthworms, although otherwise very different in character, became covered increasingly with vegetable mould, whereas, on the other hand, many plots of land not essentially unlike the former plots as a whole, but deficient in earthworms, did not get covered with vegetable mould. He therefore concluded that the vegetable mould is due to the agency of the earthworms."[3]

Now, while in the two types of inductive method illustrated above a different verbal analysis is set forth, yet it is obvious that the illustration used in the latter method is just as truly an example of concomitant variation as is the former. In the first case we say that a change in the amount of heat brings about a change in the amount of space occupied, while in the latter we say that a change in the amount of earthworms brings about a change in the amount of vegetable mould.

[2] Wolf, A., *Scientific Method*, p. 61.
[3] *Ibid.*, p. 70.

In the first instance our related variables are temperature and volume, while in the second they are worms and vegetable mould. Thus the philosophical differentiation of one method into two but serves as a barrage to obscure the essential characteristics of the scientific method. But much as this idle analysis deserves the anathema of the groping methodologist we are here primarily interested in pointing out an important characteristic which the variables involved in both illustrations have in common, namely, that of continuity. Another way of saying this is that the variables may take any quantitative value within certain limits. That is, we do not say either there is heat present or there is none, either there are worms or there are not; rather there is a certain amount of heat present or a certain number of worms, and likewise for the other variables in the respective systems.

The importance of this obvious but often neglected characteristic of a variable lies in the fact that only when it is continuous can it be treated mathematically. But this continuous character of variation has little practical significance unless it can be measured. And measurement implies the existence of a unit of measurement. Once the scientist has identified or defined the variables of the system which he wishes to investigate he must select a unit of measurement so that the variation of the attributes of the system may be quantitatively indicated.

A unit of measurement to be useful, must be stable. That is, it must retain a constant value from one time to another, and throughout the range of variation. If it does not exhibit this stability its instability must follow some known principle as time or the variable changes. The units of the physicist, such as centimeters, grams, seconds, degrees, etc., are familiar enough. The psychologist also employs temporal units, but that is about the only unit of which he can be at all confident. When he uses a test item as a unit of intelligence he has no assurance whatever that one item has the same value as another. Other devices have been resorted to in order to stabilize the unit of measurement, but rarely with any flattering degree of success. A great deal more ingenuity is required in establishing units of measurement for the variables in psychological phenomena than for those in physics. Offhand we may attribute this difference to the fact that the phenomena of the former are much more complex. The attributes of the systems investigated by the psychologist are often highly complicated, being themselves composed of more simple variables.

The psychologist should attempt to so determine a unit of measurement that for the various probable changes in the simpler undefined component variables the unit will remain relatively stable

as the composite variable changes. Often he is not aware of the necessity for imposing this condition upon the unit of measurement. To the extent that it is possible to identify and measure constituent variables of more complicated variables, to that extent the task of constructing a stable unit for the composite variable becomes less arduous. This is true, of course, because whenever a sub-variable of a composite variable is identified, that sub-variable may be detached from the complex and treated as a separate variable, while the complexity of the composite variable is reduced. The problem, then, is to determine which is the more difficult, to construct a complex stable unit with which to measure a composite variable, or to reduce the composite variable to simpler ones for which simpler units of measurement may be constructed. In this discussion we can do no more than suggest the importance of the unit of measurement, essential as may be the logic of constructing units which will remain relatively stable for composite variables.

The first step in the scientific method is the isolation of a system by the identification of the variables determining that system. The second step is the selection or construction of a unit of measurement by means of which a specific value of the variable may be spatially or quantitatively expressed. Before proceeding to the third step we shall suggest how an appreciation of the significance of the first two may greatly simplify analysis. The number of phenomena studied by the scientist may be materially reduced when the analysis is made with careful attention to variables and units. To illustrate we may refer again to another of the inductive methods known as the "Method of Difference." "If two sets of circumstances are alike in all relevant respects except that in one of them (called the Positive Instance) a certain antecedent is present and also a certain consequent, while in the other (called the Negative Instance) both are absent, then that antecedent and that consequent are related as condition and consequent, that is to say, that consequent will always follow that condition." As an illustration, "if a healthy animal is inoculated with the blood of another animal suffering from anthrax (splenic fever) and contracts the disease, then the inoculation will be regarded as a condition of the infection."[4]

This may be true enough so far as it goes. But let us raise a few questions concerning inoculation and contraction of the disease. First we ask how much blood was inoculated. Second, if one-tenth as much blood had been inoculated would the animal have contracted the disease? Third, suppose the same amount of blood had been inoculated into a second, and a third, and a fourth animal, would these all have contracted the disease? Obviously, we are here intro-

[4] Wolf, A., *Scientific Method*, p. 57.

ducing the concept of variables and units of measurement. One of our variables is volume of blood and the other is number of diseased animals. The unit of measurement for the first may be cubic centimeters or any other spatial unit, while for the second it is a single diseased animal. To say that an animal contracted splenic fever when inoculated with an arbitrary amount of infected blood is of little more importance than to say that a sparrow flew in through the window and a member of the household died sometime thereafter. But we attain a degree of scientific rigor and precision when we say that a change in the amount of blood inoculated brings about a change in the number of animals contracting the disease. And we do more, we eliminate one of the inductive methods, so that the method becomes again that of Concomitant Variation. Thus our categories are simplified.

Suppose, however, that we do not wish to vary the amount of blood inoculated. But we repeat the experiment a number of times with the same amount of blood, and find that a given number of animals contract the disease. Here we have a special case of the Method of Concomitant Variation in which the variation is zero, and we find it quite unnecessary to make a separate classification of the method.

Often in the less accurate sciences our concepts become needlessly confused and our categories unpardonably multiplied simply because we fail to take into account the fundamental importance of the concept of variation. A great deal of time and effort is spent in finding new categories and splitting up old ones, and then the classifications do not admit of satisfactory relational analysis. We exhibit a pitiable lack of insight in distinguishing between a general variable attribute and special values of that variable. Sometimes it is not easy to recognize two phenomena as separate and specific values of the same variable. But often this is due to the fact that our variables are too poorly defined, or perhaps not at all. And sometimes, even if the variable itself is adequately described, the unit of measurement is so obscure that it would be impossible to identify a given phenomenon as a particular value of that variable. A variable should be so defined and its unit of variation so determined as to include the greatest possible number of phenomena as special values of that variable. Conversely a phenomenon should be so described as to be representable as a given value of the fewest possible number of variables.

Thus far we have devoted most of our attention to a consideration of the measurement of change. The term "measurement" has assumed an almost hallowed significance for a great many investigators in social sciences. Lord Kelvin said "when you can measure

what you are speaking about and express it in numbers you know something about it, but when you can not measure it, when you can not express it in numbers, your knowledge is of a meagre and unsatisfactory kind.'' The accurate sciences are noted for the success they have had in measuring precisely their phenomena. Workers in the normative sciences have noted this success and have shown some appreciation of its importance in the progress of the physical sciences. But though the relation of successful measurement to the progress of a science should be obvious enough, much of the work in psychology would indicate that the nature of this relationship has been at times overlooked. Certainly it is not enough to measure accurately a given phenomenon, be it the light wave of a given color or some specified character trait. And let us keep in mind when we speak of measuring a phenomenon we imply nothing in the way of actually finding its objective magnitude—whatever that may mean. We mean simply that we are finding quantitative correlates for changes in that phenomenon.

The usual way of saying that science is interested in acquiring knowledge with reference to the sequential relationship of changing conditions, is that it attempts to discover general laws. Hence, a law is no more than a description of this sequential relationship. It can never be more than that. We avoid the use of the expression ''cause and effect relationships'' because this implies a necessity in the nature of the sequences which can exist only if we define necessity as a high degree of probability of the occurrence of a certain set of sequences.

But before we proceed further in our discussion of the relationship of changing conditions let us attempt to harmonize the concept of sequential relationships with that of concomitant variation. It will be remembered that of several of the inductive methods we have indicated a preference for that of concomitant variation. This preference, of course, is the usual one in contemporary scientific method. It implies, evidently, that when one variable changes some other variable changes also. And more, that these respective changes go on at the same time. When we speak of a sequence of events we imply that a change in one variable is followed by a change in another. That is, there is a time interval between the change in one variable and the change in the other. By usual convention the variable which changes first is considered the cause of the second. Apparently no one has taken the trouble to point out that concomitant variation is only a special case of sequential variation where the time interval is zero. Thus sequential variation is a more general concept than concomitant variation. In most psychological problems the nature of the formulation and the statistical

treatment assumes that the variation is concomitant. In economics sequential variation is sometimes assumed and the appropriate time interval is determined by statistical devices which we shall not go into here. We may indicate, however, that these devices are of a trial and error character, and that no systematic technique has yet been devised for determining the time interval which most adequately expresses the relationship under consideration.

Science then identifies its variables and by selecting or constructing units of measurement finds quantitative correlates for them in order to show according to what system or principle a change in one or more variables effects a change in another. When we say that it shows the change to take place according to some system or principle we have in mind certain mathematical concepts which express the type of the relationship. The simplest type of relationship possible is, of course, a linear or straight-line relationship. This means that for a change in one variable there is a proportionate change in another. Whenever the Pearson correlation coëfficient is calculated from psychological data (or any data) it is always assumed that a change in one of the variables is accompanied by a proportional change in the other. Hence, most of the research in psychology which has gone far enough to attempt to demonstrate any laws has assumed proportional variation. Another way of saying this is that most of the quantitative work in psychology assumes the simplest law possible and that the data from the work itself merely tests the validity of this assumption.

Mathematically, there are various types of relationships which may apply to psychological phenomena. One variable may vary as a given power or root of another, or as a logarithmic, trigonometric, or exponential function of the other. Or there may be complicated combinations of these various types of relationships. The determination of the particular type of relationship is one of the most difficult steps in the formulation of a general law. Once the type of relationship is determined it is easy enough to verify the degree of accuracy with which it describes the facts or data, but the data give no unequivocal clue to the nature of the relationship, expressed in terms of mathematical operations. However, the data might enable one to determine what sort of function would approximate a fit, but that is not the same thing.

One might say that the type of relationship is determined by making certain hypotheses and assumptions—usually that is the way the issue is evaded—but closer investigation will show that there are certain definite methods of approach. Sometimes the isolation of the system and the description of the variables involved will suggest the form of the relationship. Sometimes, if the relationship of

two variables to another variable is known the form of the relation between the first two may be determined. Or, in general, if the relationship of a series of variables to another series of variables is known, and relationship between the members of the second series is also known, then it is often possible to derive the form of the relation between the members of the first series. Again, it may be that the relation between specific values of certain variables is known. Then it is sometimes possible to determine the general form of the relationship. In all of these cases the hypotheses are of a mathematical form, the form being derived by mathematical manipulation and combination of simpler forms. Usually, after the development of the more complex relation assumptions are introduced which simplify it. These assumptions are of a quantitative nature, and take the form of treating inequalities as equal, or variables as constants, or small magnitudes as zero, or large magnitudes as infinity.

Perhaps one of the most fertile fields for research in methodology and at the same time one requiring the most ingenuity is that of developing and systematizing general methods of approach for the determination of types of relationships. Various specific devices have been employed by the theoretical physicists, but no satisfactory attempt has been made to organize a general method of approach.

Besides the identification of variables, the selection of units of measurement, and the determination of the type of relationship existing between the variables, there is yet one more essential step to the scientific method. That is the determination of the degree of accuracy with which the formulation describes the relationship. This is a vital part of the method, though one which has not received the emphasis it deserves. The statistician has a measure of this degree of accuracy which he calls the standard error of prediction. It is a value which can be determined when the form of the relation and the data are given.

A consideration of the significance of this measure leads us to observe that several formulations may conceivably be employed to describe the relationship between the same set of variables. Ultimately the criterion determining which formulation shall be selected is the magnitude of the standard error of prediction. In actual practice the standard error of prediction will never be zero. But the more accurate the formulation and the more precise the measurements the smaller will be this error.

It should be noted that throughout our discussion nothing has been said about the experimental method. Often it is assumed that the experimental and the scientific method are the same. The experimental method is but an aid to the scientific method, experi-

mentation being only incident to the accumulation of data to be used in the method. Nor is control an essential characteristic of the method. Often it is stated that when the scientific method is employed one variable is varied while all the others are kept constant. The scientific method is much more general than this. If it were not it could not be employed by the economist or the astronomer. It is not necessary to be able to vary a given attribute at will nor to hold all others constant. It is necessary, however, that the variation which takes place be noted, and this is just what happens when the economist gets his data from industrial and government records. It is the record of the variations and not their control which is prerequisite to the use of the scientific method. The advantage of experimental control is that the data may often be more easily obtained and may be more adequate for purposes of analysis.

PAUL HORST.

WASHINGTON, D. C.

ARE WE MACHINES? AND WHAT OF IT?[1]

IT is one of the curiosities of human nature that great numbers of men and women are apt to become violently excited over the question whether or not they are machines. By all the tests in the psychologists' repertoire these men and women are intelligent enough to be allowed at liberty and entirely competent to conduct the ordinary business of civilized society. Indeed it is usually the more intelligent who are particularly concerned. Yet they seem haunted by a terror that they may suddenly awake to find themselves hopelessly entangled with gears and belts and fly-wheels and vacuum tubes.

It is a curiosity, amusing to a philosopher, and nothing more, because the question is really not at all serious or important. There *are* possibilities about which it is right to be seriously perturbed; but this is not one of them. Any man is justified in being deeply agitated over the bare possibility that he is heir to a fortune, or even that an ancestor fought in the Revolution or occupied a stateroom on the *Mayflower*. These things make a difference. Something is at stake. Life will be one thing if our individual's hopes are realized, another if they are disappointed. But whatever we may finally decide is our relation to gas engines and dynamos, life will remain exactly what it was before the question occurred to us. The whole problem, indeed, is only a little more important and interesting than this other—Is a pretty girl a peach?

[1] Read before the Fortnightly Club of Redlands, Feb. 5, 1931.

This must be either a very profound and startling observation or else very absurd and foolish. Assertions with which the mass of mankind is in violent disagreement are usually one or the other of these extremes; and this is one of such assertions. It would almost certainly occur to a philosopher that, if human beings are machines, this is nothing new. They probably always have been. The belated recognition of the truth could not change them in any way. Especially it could not make them worse or less than they were. It might lead to a more intelligent lubrication of some of the bearings or a readjustment of some of the loads. All that might be ground for a mild optimism.

But there are lamentably few philosophers. Other human beings worry. To most of them it is plain that the doctrine would be ruinous. Let men once be convinced that they are machines, and over the whole wide world things will never be the same again. The pillars of society will crumble. Morality will collapse. Life will inevitably lose all joy, interest, dignity, or worth.

To a few "Mechanism" is a gospel. In their utterances a humorist can discern a curious reversed evangelism. It has its Billy Sundays and doubtless its Aimee Semple McPhersons. Salvation by Science is proclaimed with jubilant war whoops that lack only a sawdust trail or a sprawling "tabernacle" to recall the old-fashioned "revival."

Dr. Watson, to pick a single shining and amusing example, even outdoes Billy Sunday. With a truly medieval fervor he announces that the Day of the Lord is at hand. He chants a *Dies Irae*, as he contemplates the doom of all other than Behaviorist modes of thought. The day of wrath for the unsaved, however, will be for the elect Behaviorists "Glory for Me." It will be heralded not by trumpets of archangels, but by notices in the press of the endowment of immense laboratories to study how children may be made afraid of rabbits. The Empire of Science is about to swallow up the Kingdoms of the Gods. That is progress—as a Union Station is an improvement on the Parthenon.

It is almost a pity that, in spite of so much fuss and fury, nothing of any real importance is at stake. Whether we call ourselves machines, organisms, or souls, or something else, we are just what we are. Practical problems remain just what they were. Theoretical perplexities persist in as baffling and challenging forms as ever.

Dreadful results *may* follow the acceptance of "Mechanism." But that will be only because we fly into a panic over it. In that event we can blame only the panic for the disaster. On the other hand glorious advances may follow. But again, the doctrine will not deserve credit. We need more laboratories; but "rational

souls'' or ''thinking machines'' are equally capable of endowing them. All that is needed is that the souls should be rational or that the machines should think. I personally think that a soul would be somewhat *more* likely to endow a laboratory than the most thoughtful of machines. But it is the thinking that is important, the facing of facts, the refusal to fall either into panic or hallelujahs.

A reader of this paragraph may be wondering whether he should marry Rose or Marie. He may even contemplate marrying both! Or should he pay his creditors or take refuge in bankruptcy? Yet again, he may be wondering whether he should buy government bonds or oil stock. These are typical practical problems. Now let him be persuaded that he is a machine. Will he buy the speculative stock, defraud his creditors, or risk the legal and emotional perils of bigamy? Not unless, in addition to being a machine, he is a foolish or dishonest one.

Let a man be as mechanical as the most fanatical Behaviorist (scientific Fundamentalist) might wish him to be. Still he is a machine that is capable of happiness or misery. Moreover he is capable of producing happiness or misery in other mechanisms. At every crisis he must face the question whether, for the kind of a machine he is, one line of operations or another is the better. Machine or soul, his practical problems are the same.

Theoretical problems are no more helpfully illuminated. Assume once more that human beings are machines. Still, they are not locomotives, nor lathes, nor radio sets. They are not cattle. They are not even apes. They are machines of a particular kind. What kind? Press the question, and the answer sooner or later must be—human machines. But what distinguishes human machines from those that are not human? What constitutes humanity? Here we are back where we started. A human machine is just as different from a typewriter or a chimpanzee as a human soul would be—and different in precisely the same ways.

With disarming candor and in a most ingratiating style the distinguished neurologist, Professor Herrick, in his recent interesting volume, *The Thinking Machine,* contends that we have conceived machinery too narrowly and crudely. We have thought machines were only such things as automobiles and printing presses. If we will only enlarge our conception of machinery ''to fit the larger task,'' we shall see that human beings may be machines and yet be human, that the brain may be a machine and yet really think. To help us enlarge our concept he offers us a new definition of a machine. It is, he says, ''a material structure that performs work by natural forces, that moves matter and energy against resistance, that gets its material and energy from natural sources, and that delivers natural products'' (p. 22).

If this is all that Professor Herrick means by a machine, no one has any occasion to object to being called one. Unfortunately Professor Herrick leaves nothing in the world but machines. He might as well have said, "By machines I mean anything at all." When a term means anything at all, it means nothing in particular. To say that human beings are machines in the sense we are discussing, is simply to say that they live and move in constant interaction with a world of matter and energy. Who would want to argue otherwise?

A humorist can make the matter clearer than a philosopher. Let us construct a parallel argument to show that pretty girls may be peaches. We will admit at the outset that this is a new and disturbing idea, that most people do not believe them peaches, and that grave anxiety may develop as the discussion proceeds. But is not all that because he have had altogether too crude and inadequate ideas of peaches? We have been accustomed to think of them only as fruits that grow on trees at a certain time of the year. But if we enlarge our conception of peaches, all anxieties and objections become groundless. We have only to define peaches in such a way as to include two-legged creatures that walk around at all times of the year, wear skirts, and use lip-sticks. Once we have so expanded the idea, we shall glimpse a profound, indeed a fundamental truth. And we may find a publisher, and even a market, for a book upon our discovery.

When we call human beings machines, a girl a peach, an automobile a boat, the fish that got away a whale—in general, when we call a thing anything but just what it is, we employ a figure of speech. And if we sternly resolve to eschew all metaphors and call things only just what they are, we find ourselves limited to such solemn pronouncements as that "A man is a man," "A girl is a girl." We achieve an unimpeachable veracity, but our remarks are neither interesting nor illuminating.

Metaphors are possible, because two objects are similar in some respects; they are interesting, because the two are strikingly different. A pretty girl does make anyone with the least esthetic susceptibility think of a peach. A real and considerable satisfaction results from making the comparison. But that is precisely because she is not a peach.

When we assert that human beings are machines, we are utilizing a metaphor. It is true or false in exactly the same ways that it may be true or false that a girl is a peach. I do not mean to insinuate that the mechanism metaphor is no more valuable or illuminating than the peach metaphor. It probably is both more valuable and more enlightening, more stimulating to thought, more productive of important discoveries and improvements. But for all that, it is a metaphor.

So we have only to appraise a metaphor. Nothing more serious is at stake. Humanity is in no danger of losing any important characteristic. Nor on the other hand is humanity to be secured in the possession of any particular prerogative. We may investigate the ways in which human beings resemble steam shovels and cash registers with no need to worry about the destiny of the race. Whether we call them machines, organisms, or souls, human beings are human.

II

Perhaps it is impossible, certainly it is difficult, to define a machine. So capable a thinker as Bertrand Russell, in his debate with Professor Mather, declared, "I do not know what a machine is." It is easy, however, to point out certain fairly definite characteristics of the machines we know and use. And we may classify them as positive and negative meanings of "mechanical" or "mechanism." Machines have certain qualities and lack others. Certain adjectives can be applied to them and certain others can not.

To begin with the positive meanings, machines as contrasted with animals and plants are (1) accurate, (2) regular, (3) and dependable. In all machines, too, there is (4) a certain transparency of relationships. The reason for every movement is at once apparent to anyone with the capacity to perceive it and in the position to do so.

This last is emphatically not true of plants and animals. We may believe that *if* we knew more, we should find their actions as transparent as those of our sewing machines or gas meters. But that is faith. Scientists and Mechanists have as much right to faith as any others—probably more. But neither they nor the religiously devout have any right to call faith knowledge. For the present it is certain that we understand, "grasp," or "see through" our machines, but that the behavior of even so simple an organism as an amoeba is in most respects a baffling mystery.

All these positive meanings are complimentary to human nature, or at least unobjectionable. It is acceptable flattery to speak of a football team as a smoothly working machine, of a runner's machine-like stride, or of the mechanical perfection of some athlete's "form." A bookkeeper or an accountant is entitled to solid satisfaction, when the correctness of his additions can be compared with that of an adding machine. And we may recall that his fellow townsmen used to set their watches by the philosopher Kant.

Accuracy, dependability, efficiency, are highly prized human traits. If to be mechanical meant nothing else, there would be no scandal, no controversy, no anguish of heart. We might even go to the other extreme; and Society, when it sought to confer its supreme

honor upon some noble personality might devise a new order of nobility of higher dignity than those of the Bath or the Garter—an Honorable Order of Machines.

Transparency, as we have applied the term to machines, may seem a dubious compliment. Psychiatrists are apt to speak of "mechanisms" of character or conduct, whenever they are able to discern any intelligible system or orderly development in the behavior of their patients. Popular fancy tends to invest the psychiatrists with something of the gruesome awe that in the middle ages attended the reputed wizard or the torturer; and the thought of psychological "mechanisms" may arouse a shiver—if not of horror, at least of repugnance.

But the opposite of transparent is opaque. As we have been using terms, that means unintelligible. Why should we wish human conduct or experience to be that? There is nothing noble or admirable in caprice or chaos, in human lives without principles. On the contrary, to be human is to be forever struggling to reduce the chaos of Nature and of our own natures to order and system. In the proportion that this is accomplished, conduct becomes intelligible. In becoming "mechanized" in this sense, it gains rather than loses in dignity. Only a perverse mind can insist that human behavior becomes degenerate as it becomes intelligible.

So far, then, as the positive meanings of "mechanical," "mechanism," or "machine" are concerned, we may conclude that they are high human ideals. Only in exceptionally favorable circumstances do human beings approximate the excellencies of machines. To be "mechanical" in any positive sense is to be fully, ideally, human.

It is altogether different with the negative meanings. When a process is said to be "mechanical" at least five distinct denials are involved. (1) A machine acts without knowledge of itself, its uses, or the effects of its operation. (2) It has no purposes; it does nothing for any conscious reason or to any self-selected end. (3) It has no feelings, neither happiness or misery, no love, hate, or pity. (4) It can not act save in a rigidly determined fashion. And (5) it is unable to adjust itself to changed circumstances.

These negative characteristics of machines are all sub-human. That is why human beings object to being called machines. Any intelligent person will resent an attempt to label him an imbecile. And there is even more reason to resent and repudiate any description of mankind which does not acknowledge and provide for humanity.

It will not help the mechanists' argument to point out that human beings do occasionally act ignorantly, brutally, stupidly, or monot-

onously. It would not help, even if it could be proved that they generally act so. For men are at least sometimes human. Machines never are. However rare a phenomenon genuine humanity may be, it is distinct from everything that is sub-human. To confuse it with anything of a lower order is simply evidence of muddled thinking.

We must admit that it is difficult to explain purposive or conscious behavior in terms of a mechanistic physiology. That is no reason for denying an obvious and familiar fact. It *is* reason to suspect that our present physiology, and indeed all our present science, is incomplete and many of its conclusions premature. The procedure of true scientists will be to elaborate and refine our physiological, and even physical, concepts until it is possible to provide for such activity. Then we shall have a physiology of conscious, purposing, feeling human beings, not of hypothetical "mechanisms."

And if ever purposive behavior is "explained," no one need feel dismay or disillusionment. It will not be less purposive, less intelligent, or less conscious than it was before it was understood. To "explain" is merely to exhibit the relations in which one fact or event stands to other events or facts. That means enriched insight. If it seems to mean the loss of the fact, something is wrong with the "explanation."

Mechanists are obnoxious, because they are continually insinuating, perhaps unintentionally, that human beings are not human. The instant we see the matter in this light, we can see also that there can be no just ground for anxiety. We can indeed group men with machines in our thinking. There may be some advantages in doing so, as we have already seen. But to do it, we must strip them, in our thinking again, of their distinctively human characteristics. It is as though we were to strip them of their flesh, muscles, and viscera. The one process yields us machines, the other skeletons. Both are legitimate and useful in appropriate circumstances; but neither gives us human beings in their entirety and the rich variety of their full activity.

III

Mechanists usually, however, when they speak of machines do not mean machines. They could not stick to a legitimate use of the term and escape obvious absurdity. What they do mean was probably well expressed by Bertrand Russell in his debate with Professor Kirtley Mather in Chicago in November of 1929.

"I do not know what a machine is. . . . When I say that men are machines, I mean that their bodies obey the laws of physics and chemistry, and that those laws are adequate to explain all their behavior. . . . I do not know what those laws are. . . . Sometime, when we know more, we may understand them."

In spite of the fact that two or three thousand people paid good money to hear it, it is doubtful whether that statement means anything at all. At most it amounts to two simple and unobjectionable propositions. The first is that men live and function within a world of things. The other, that human behavior, once it is understood, will be found to stand in systematic relation with all other events and processes. The first is a truism. The second is a pious hope.

Mechanists have as much right as any others to pious hopes. But this particular one is no exclusive property of theirs. It is just as strongly held by their opponents. Who can object to the faith in order and system? Only those who for some perverse reason set a premium upon caprice, discontinuity, and disorder generally.

Mr. Russell really surrendered the case for "mechanism," when he admitted that our present laws are inadequate for the explanation of even the simplest facts of human activity. No intelligent person objects to the idea that human behavior and experience can be explained according to *some* laws. But every intelligent person does, and must, object to the assertion that our present physics and chemistry can fully do so.

IV

An interesting contrast is to be observed in the attitude of the "intelligent layman" and the scientist toward scientific or natural "laws." The layman is apt to carry over much of the awe that in his childhood he used to feel for those mighty edicts enunciated amid the thunders at Horeb. The scientists know—and by this time they ought to, for the philosophers have long been telling the world—that their laws are promulgated from no more awful Sinais than scientific laboratories; their tables of stone are only scientific journals. The authors are no deities but men who may have false teeth and flat feet like the rest of us. The laws are not mandates ordering the universe to obey traffic signals. They do not coerce in any way the events or activities of Nature. They are merely reports of how things have so far been observed to act. The layman is apt to speak of their "unalterable majesty and inviolability." The sound is fine but the meaning is nonsense. The "laws" are always being broken. Or, less figuratively, they are always being found incomplete and premature. When that happens, the disconcerting fact is not put in jail, or even on probation. The "law" is amended.

That is how science progresses. Whatever may be the difficulties with the metaphor, science is not built upon "solid foundations of eternal verities." On the contrary its towering structure rests upon the debris of discarded theories; and its inspiring growth is possible because its foundations and every one of its members are forever crumbling.

The value of any "law" depends upon the range of facts which it summarizes. If "laws" are derived from a limited field of events, it is absurd to expect them to hold unchanged when wider fields are explored. "Laws" which may suffice to describe the movements of planets, the behavior of chemicals in test tubes, or the operation of an aeroplane, have been derived from just such events. Their validity is limited to the events from which they have been derived.

What of human behavior and experience? Were these among the data from which the Law of Gravitation, or Einstein's equations, or Mendel's Law were deduced? They certainly were not. Are we to conclude that behavior and experience are only special cases of much more general formulae? That is perhaps conceivable; but it is violently opposed to all probabilities.

In the science of the future human activities will have a place. In all probability "laws" will be formulated that will enable the happy beings of that time to predict and control and understand the whole course of human life. But that time is not this time. That science will be a very different science from the one which today is only just beginning to chart the coast line of personality. It will be as different from the science of our time as geography today is different from that which was taught before the Americas were discovered, or as modern biology is different from that before Darwin and the general acceptance of evolution.

V

Why are we, so many of us at least, so afraid of science? We seem to fear that from the tangled wires and gleaming brass instruments of the laboratories demons will emerge. As the devils of old robbed men of their shadows or their souls, these modern demons will rob them of their humanity.

It is easy to understand such a terror in the heart of a medieval scholar. It is easy to understand and sympathize with it in a Fundamentalist. But many of the loudest eulogists of science have essentially the same attitude. In rather too gorgeous rhetoric Bertrand Russell laments that science has at last revealed man as only an "accidental collocation of atoms." Did ever a medieval or rabbinical exorcism employ a more mouth-filling and horrific appellation! Ayres calls science a "False Messiah." A host of literary imps are assuring us that the demons have already done their work. Only our invincible "booberie" or a frantic clinging to quickly fading dreams keeps us from realizing our misery and our degradation.

Why is there such timidity in facing facts, such snickering glee at awful discoveries? Why do so many of us dread that the truth, when once we know it, will be *bitter?*

Is it because in our childhood our curiosity was unwisely repressed? Were too many of us frightened with tales of bears and ghosts and bad men and skeletons in dark closets? Were our early inquiries into the mysteries of sex repelled with such horror that *all* investigation was shrouded with superstitious horror?

Psychoanalysts and psychologists are invited to make what use they wish of these suggestions. But why should we not expect that truth will be *sweet?* It is more in keeping with all the probabilities to expect that man will not be degraded by being linked to Nature in scientific system; but that on the contrary Nature will be illuminated and ennobled by the study of man.

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BOOK REVIEWS

The Nature of Life. EUGENIO RIGNANO. (International Library of Psychology, Philosophy and Scientific Method.) New York: Harcourt, Brace & Company. 1930. Pp. x + 168.

The first part of the book argues that purposiveness is essential to living organisms; the second part sets forth, as mediating between vitalism and mechanism, the author's hypothesis in explanation of this purposiveness. As he states it: "we shall try to prove that our hypothesis as to the nature of life, which we have developed in our preceding works *On the Inheritance of Acquired Characters* and *Biological Memory,* is fully adequate to explain these purposive manifestations" (Introduction, p. x). This hypothesis lies "between the two opposing theories of vitalism and mechanism which, till now, have held the field alone" and "it explains the purposiveness of life on causal and deterministic lines" (p. x).

The nine chapters of Part I are devoted to showing the essentially purposive character of the chief organic processes. Assimilation is a selection of certain substances from the complex mixture taken in as food, and a synthesis of these into the compounds specific for each organism. By this synthesis the organism "reconstructs exactly and exclusively the selfsame substance which was destroyed during the particular process of dissimilation which it has to repair" (p. 6). This rhythm whereby the living body is regularly restored to the "dynamic stationary equilibrium" (p. 8) is never found in the inorganic. Thus life appears as "an energetic synthesizing process *sui generis*" (p. 14); it is "a new form of energy—always obedient to the general laws of energetics, but endowed with well-defined elementary properties different from those of any form of

energy in the inorganic world'' (p. 14). This energy, the *differentia* of life, the author calls nervous energy; it is indeed ''nothing other than nervous energy properly so-called, even when it is produced by the nuclear discharges of non-nervous cells, whether animal or vegetable, somatic or germinal'' (p. 34). Metabolism is constituted by the fact that ''the discharge of nervous energy caused by the disintegration of the living substance would immediately be converted into a charging current which would reproduce in identical quality and quantity the living substance destroyed in the immediately preceding phase'' (pp. 18–19). Of course the critical reader will here ask why the energy possesses this rhythmic and restorative function; but Professor Rignano seems to regard the function as an ultimate fact. Certainly if nervous energy should turn out to be, as some think, an electrical phenomenon, the way would seem to be open to the mechanist.

To continue the argument of the book: the fact that the organism in early stages ''forms in itself organs which will *later* serve for its conservation'' (p. 24) is clearly purposive. There seems here to be a ''predetermined end'' (pp. 24, 25); and this fits well into the hypothesis of nervous energy. ''According to our hypothesis the specificity of nervous current or discharge which is produced by the sudden decomposition of a given nuclear substance is the same which, functioning later as a current of charge, reproduces the same specific substance'' (p. 32). Thus it would seem (the text is too summary to be quite clear) that the rhythmic return which was assigned to metabolism accounts for the repetition in offspring of the parental structure and behavior. This return to the original equilibrium and pattern is a mnemonic phenomenon (in Semon's sense): ''all the specific accumulations, deposited both in the nuclei of the very varied somatic cells of the different tissues and in the germinal cells are really authentic mnemonic accumulations, fundamentally of the same nature as those deposited in the centres of the grey matter of the brain, which produce by their discharge the psychic phenomena of mnemonic recollection in the strict sense'' (pp. 34–35). Unfortunately the detailed argument is not here given; we are referred to *On the Inheritance of Acquired Characters.* Meanwhile we may note that what is called pure memory is not accounted for in this way; pure memory is no mere repetition of the same discharge, but is the awareness of repetition. In fact it is not clear whether or not nervous energy has a tincture of awareness. The author later says ''Again we have not, with Neumeister, been compelled to introduce any sort of 'psychical thinking' into the chain of physiological phenomena. . . . To postulate at the basis of life such a 'psychical thinking' of which neither the nature nor

the origin nor the method of functioning is known and which is an unknown much greater than the facts which it is supposed to explain, is to fall into pure metaphysics and to renounce all positive scientific explanation" (p. 132). Yet in the next sentence he speaks of this "special form of energy, endowed with a special, essentially elementary and well-defined property such as that of specific accumulation, from which can be deduced all the purposive manifestations of life, including those of the thinking psyche which are characteristic of mind" (p. 132). Again: "Now this 'faint glimmering of the human psyche' whose presence we must recognize in the smallest fragment of living matter, is nothing else than the special form of energy which we assume as the basis of life . . ." (p. 133). Is the nervous energy then conscious or not? Or is it neither, but a potency which develops into one or both? The only answer I find is on pages 98–99: "*Consciousness,* which so far has constituted the greatest enigma of psychical life, appears to us not as an intrinsic and absolute property inherent in each of the various psychical states, considered apart from the others, but a property extrinsic and relative to each one of them, due to certain modalities of affective reference existing between them." If the reader understands this, he has the advantage of me. It would be better "to fall into pure metaphysics," at least for clarity's sake.

In the remainder of Part I the purposive traits of life are emphasized. Adaptation has not been sufficiently explained by natural selection. Behavior of the lower organisms shows trial and error, but "to try implies an end to be attained" (p. 81). Of course the difficult problem of instinct is rendered less difficult if one accepts with the author the inheritance of acquired traits, but probably most biologists will demur. For the author "the instincts appear to be of an essentially mnemonic nature" (p. 88). Finally, purposiveness obviously belongs to mental activity and social conduct.

Having argued that purposiveness remains inexplicable on physico-chemical grounds, Professor Rignano in Part II shows how his hypothesis mediates between the two extremes of mechanism and vitalism. Accepting teleology, it offers, as vitalism does not, a definite and in a sense verifiable hypothesis; for nervous energy is an identifiable process. This I think the critic must grant; but the position now looks very like materialism (which here is mechanism). We read: "intelligence, will, and the complex working of the mind in general . . . have their own spatial seat, their own material basis, their own energetic potentiality" (p. 127). After all, nervous energy is a purely physical affair; no amount of repetition or cyclical rhythm can turn it into a mental one. He compares it indeed in Chapter II to the ether or to electricity, which are new

forms from the point of view of the laws of motion. But once more the critic must put the dilemma: either this nervous energy is conscious (and then we have straight vitalism) or it is not (and then we have straight mechanism). There is no third possibility. There is no way of mediating between mechanism and vitalism. Monistic views such as those of Haeckel, Spencer, Avenarius, and the present author, solve the dualism by ignoring it.

The book is concluded with a chapter on "Man's Place in the Universe and the New Morality of the Harmony of Life," which urges that the hypothesis of nervous energy justifies the virtues of social coöperation and the aspiration of man toward finer and nobler human life.

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The Biological Basis of Human Nature. H. S. JENNINGS. New York: W. W. Norton and Co. 1930. xix + 384 pp.

Professor Jennings has given us the completest summary of the relative importance of "nature and nurture" in the integration of personality yet published from the biological standpoint. Try as he will to be impartial in his summary of the evidence, this book is still a defense of the waning prestige of the old biological dogmas that half a generation ago reigned supreme through their spokesmen —C. B. Davenport, H. H. Goddard, M. F. Guyer, and many others. At that time it was the sociologists against the biological field, with scarcely a deserter from the biological camp. Only H. W. Conn among the biologists had discovered the fallacy of the orthodox hereditarian argument and had proclaimed it in his *Social Heredity and Social Evolution.* E. G. Conklin was beginning to compromise. Several years later Goddard, under the influence of wider contacts, greatly modified his views. In 1924 Jennings, and in 1925 Davenport, published articles in the *Scientific Monthly* acknowledging the overstatement of the orthodox hereditarian position. And now Jennings sums it all up, "in the light of the latest facts," and with a note of apology for what survives. One thing is very notable—for its absence—in this whole intellectual warfare between the sociologists and the biologists. The biologists, nowhere and in no way and never, have given any adequate recognition by name, title, or idea to the work of the sociologists who literally forced them to restudy the evidence and to revise their views on the relative importance of heredity and environment. Yet it is evident that they know of the sociological viewpoint, for they refer to it anonymously—and frequently. In Jennings the reference is to the viewpoint of both the sociologists and the behaviorists, whom he considers environmen-

talists, and he actually refers by name to Watson, who appears to symbolize for him the behaviorist standpoint. Yet it is quite true that Watson himself abandoned his old instinctivist viewpoint after the sociologists had begun their criticism of the hereditarian dogma.

So much for the setting from which the present book grew. Although popular in form and the result in part of the republication of previous essays, it is systematic in presentation and one may learn from its delightfully written pages the essentials of the latest knowledge which the biologists have to bring to the controversy. For controversy it *is*, and this book is controversial. The new discoveries about the genes are here presented, and the author shows why unit character inheritance is not the rule. He also explains why two feeble-minded parents can have normal children, a thing the hereditarian loyalists of half a generation ago said was impossible. Jennings really does not go much beyond the admission made by Conklin seventeen years ago about environmental influence when he maintains that either the genes or the environment may modify any general trait and that ordinarily it is not possible to determine which is responsible. He gives more weight to the endocrines, because more is known about them; but repeatedly he speaks of the endocrines as being determined by the genes. He admits the influence of vitamins and seems less definitely to imply that even the endocrines may be modified from without. He creates not a little confusion in the thought of some of his readers, and possibly in his own mind, by failing to distinguish adequately between the indirect general and the direct specific influence of the genes upon the development of somatic traits. Probably not even the most rabid environmentalist has ever denied that the genes are a powerful background factor in the development of acquired characteristics as well as the specific determiners of inherited traits. All environmentalists will grant, as a working hypothesis at least, that environment works on a genic or hereditary base. But they contend that having good genes merely gives to the individual the foundation on which specific mental, moral, and social traits may be erected, provided the environment is so organized as to condition these traits in the individual. They hold that the genes are not themselves able to control the development of such traits. Jennings' failure to distinguish between the specific or direct and indirect or general contributions of the genes to personality development leaves his position as much confused as that of some of the sociologists of thirty years ago. These could not distinguish between the concepts of the hereditary or "born" criminal and of the criminal who was so because of constitutional hereditary defect. (It is not always certain now that even the constitution is inherited!) Most, if not all, sociologists

can now make this distinction. Can not the biologists and psychologists also? The environmentalist certainly would agree with Jennings that "one may show a better knowledge of Latin syntax in consequence of an original difference in the genes" (p. 164), without thereby being led to suppose that the Latin scholar had inherited his knowledge of Latin syntax. The important thing—if a knowledge of Latin syntax is important—is to know where one may secure a knowledge of Latin syntax. Is not the environmentalist safe in assuming that the method is not to be born again physically of other parents, but to seek a teacher and an environment with facilities for Latin syntax study?

Four problems of application confront the author with especial force. He admits little hope from the old program of eugenics, largely because of the difficulty of locating the bad recessive genes. While feeble-minded parents produce one feeble-minded offspring, the great masses of the population who are not feeble-minded produce thirty. Besides, he shows, it would require 68 generations, or 2000 to 3000 years, to reduce the chances of the inheritance of feeble-mindedness to a proportion of 1 to 1000 births under control. But he would not give up all hope of selective breeding, especially among recognized "carriers," and through the location of single-pair defects. Likewise, he does not believe the family will cease to have an indispensable function in society, both biological and social in character. The fact that the insects have substituted group care for family nurture is based on an entirely different set of circumstances from those that obtain in our society. He has an excellent—a statistical—definition of race, as merely the massing of similar types of genes in a population. This is essentially the definition the sociologists have used for a long time. In fact, in all three of these applicational problems, Jennings has come over entirely to the established sociological standpoints, as any reader who knows both fields may readily determine for himself. But when he passes to the problem of emergent evolution, one wonders if he is not here more of a metaphysician than an experimentalist, positing a dualism of mind and matter that does not exist. He states his position with those who oppose the "mechanist" or bio-physical basis of explanation of the emergence of mental, moral, and social traits in society on the ground that such traits can not arise from a mechanical order of the universe. It might be better to attack this question without doctrinaire presuppositions.

It may, however, be said that one may find in this book the best existing statement of the modified position of the hereditarians, still slightly confused in its analysis and terminology, but on the whole gratifyingly clear. But he will not discover in it any ade-

quate clue to where he may find the environmentalist or "sociological" views which it controverts.

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The Person of Evolution. Studies of Instinct as Contributions to a Philosophy of Evolution. W. D. LIGHTHALL. Montreal: H. A. Kennedy & Co. 1930. Pp. 216.

This little book adds to the literary treasures of the universe a new philosophy of evolution. It is an ingenious but sketchy argument for the hypothesis of Superpersonality. Treading lightly and airily the vast ranges of cosmic evolution, Dr. Lighthall everywhere discerns the footprints of that "Directive Power" and "Outer Consciousness," the Person of Evolution. First, he establishes the Independence of Instinct as "a single element coterminous with all the reactions of living substance, including their psychical concomitants." A second and correlative postulate is: "the law of action towards joy and in avoidance of pain." In this cosmic law, we find a new key to the baffling problem of teleology, namely, the persistent production of joy in nature. "Nothing is more striking in advancing biology than this persistence through unimaginable intricacies: Nature, within the body, sometimes uses a chemical, sometimes a nerve, or other device, or even invention of thought, to pursue its end, and its ends are always resolvable into joy." This increase of joy and conquest of pain, and not mere contrivance or even cosmic Order, is the true basis of teleology. Thus is Schopenhauer's argument inverted into an evolutionary optimism. The next postulate is the "axiom that every conscious unit has a power of coalescing its consciousness with, and of decoalescing it from, that of other conscious units or groups, under certain conditions." The author is well aware of the difficulties of this "colonial view" of individuality, but presents copious inductive evidence from "the snowy intricately woven tubes of *Euplectella speciosa*" and the "sparkling crystals of calcite embedded in fossil shells" to the marvellous interglandular directorate of the human organism and its sensitiveness to the waves, rays, and rhythms of the whole universe. "The beauty of the woods, the stars, the hiding wild flowers, the innumerable snow crystals, the ocean corals" are among the countless expressions of the affective harmony which forms the "daylight view" of the universe. Transcending all imperfect analogies between organism and society is the universal organicity of life and nature.

The result is a superficially brilliant eclecticism, displaying a critical acquaintance with the writings of Spencer, Butler, Kidd, Durkheim, Bergson, Hobhouse, Whitehead, Smuts, Jennings, and

practically the whole literature of evolutionary teleology. Unfortunately, the author's facility in showing how his own theory diverges from these authorities and what it is not, far exceeds his own fertility of construction. To be sure, the freshness of undisciplined synthesis is sometimes a relief from the wearying subtleties of the school-trained philosopher. Moreover, the critic's complex is to be more exacting of others' theories than his own. But when all is said, one can only regret that Dr. Lighthall did not spend ten more years out of the evolutionary eternity patiently evolving his rich teleological insights into a rigorously logical argument. Ever pertinent is Pascal's dictum that if he had had more time, he would have written less.

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NOTES AND NEWS

Dr. Houston Peterson, lecturer in philosophy at Columbia University, was awarded the Butler Medal in silver for his book *The Melody of Chaos*. The Butler Medal is awarded annually in silver or bronze by Columbia University to that graduate of Columbia University who has, during the year preceding, shown the most competence in philosophy or in educational theory, practice, or administration, or who has during the time made the most important contribution to any of these.

The Ninth Annual Congress of the Australasian Association of Psychology and Philosophy, with the coöperation of the Melbourne University Philosophical Society, met May 21 and 22, 1931, at the University of Melbourne.

Among the papers read were the following: Presidential Address—"Science and Society" by Professor John Anderson; "Explanation" by Professor H. Tasman Lovell; "Realist Psychology" by G. F. McIntosh; "Value" by Professor W. R. Boyce Gibson; "The Inheritance of Acquired Characters" by Professor W. E. Agar.